BOSTON MEDICAL AND SURGICAL JOURNAL

HIPPOCRATES IN TEMPLO ÆSCULAPII TABULAS VOTIVAS EXSCRIBENS.

VOL. VIII.] Wednesday, July 3, 1833. [NO. 21.

ON THE QUANTITY OF FOOD AND SECRETIONS IN HEALTH.

***A series of Experiments on the quantity of Food taken by a person in health, compared with the quantity of the different Secretions during the same period; with chemical Remarks on the several Articles.* By John Dalton, F.R.S.**

During my residence at Kendal, nearly forty years ago, I had at one time an inclination to the study of medicine, with a view to future practice in the medical profession. It was on this account chiefly, but partly from my own personal interest in knowing the causes of disease and of health, that I was prompted to make such investigation into the animal economy, as my circumstances and situation at the time would allow. I had met with some account of Sanctorius's weighing chair, and of his finding the quantity of insensible perspiration compared with the quantity of aliment; and it occurred to me that the differences of constitution and of climate, might occasion very considerable modifications which it would be desirable to ascertain. The following train of experiments were accordingly instituted for the purpose. It may be proper to observe that my habits, daily occupations, and manner of living, were exceedingly regular; my health during the time was uniform and good; and that the weight of my person has never been subject to much change since grown to maturity.

The first series of experiments was made in the month of March, for fourteen days successively. I had three meals each day, breakfast between seven and eight in the morning, dinner between twelve and one, and supper about seven in the evening Except on two days in which I had tea to breakfast, and again in the afternoon, the usual breakfasts consisted of boiled milk with bread and a little oatmeal, and suppers of the same, with the addition of bread, cheese and beer. The dinners consisted of butcher's meat, potatoes, pies, puddings, bread and cheese. About one-third part of the bread used consisted of a thin oatcake common in Westmoreland and Cumberland. I drank no water, seldom wine, and no fermented liquor, except common table beer. The weight of the individual articles was taken at each meal separately, and entered in a journal, distinguishing fluids from solids.

It will be quite unnecessary to give a detail of the articles and their weights just as they were entered in the journal, because it would be found little more than a repetition of names and quantities. A very short time showed that the daily demand for food, both solid and fluid, was nearly uniform as to quantity; and that the supply might have been made absolutely so without any inconvenience. But the diurnal evacuations were by no means so near uniformity.

An aggregate of the articles of food consumed in the fourteen days is given below; and the mean proportions for one day are also given, neglecting small fractions.

	Consump. in 14 days. *oz. avoird.*	Consump. in 1 day. *oz. avoird.*
Bread,	163	12
Oat-cake,	79	6
Oat-meal,	12	1
Butcher's Meat,	54 1-2	4
Potatoes,	130	9
Pastry,	55	4
Cheese,	32	2
Total,	525 1-2 Solids.	38 Solids.

	Consump. in 14 days. *oz. avoird.*	Consump. in 1 day. *oz. avoird.*
Milk,	435 1-2	31
Beer,	230	16 1-2
Tea,	76	5 1-2
Total,	741 1-2 Fluids.	53 Fluids.

Thus it appears that the average daily consumption of solid and fluid articles was 91 ounces, or a little short of 6 pounds avoirdupois. The distribution of the aliments into solids and fluids as above, is evidently to be understood in a popular sense; as it is well known that all the solids contain a greater or less portion of water, and all the fluids a greater or less portion of solid matter. In fact, water must be considered as the basis of all the fluids. During all this period, a daily register was kept of the urinary secretion and of the evacuation of the bowels. The total quantity of urine for the fourteen days was 680 ounces; and the total quantity of fæces was 68 ounces. The daily average was, urine 48½ oz.—Fæces 5 oz., a greater disproportion than was anticipated, being nearly in the relation of 10 to 1; they amount together to 53½ oz. or 3½ lbs. nearly; but the quantity of food taken daily was 91 ounces; there remains a balance 37½ oz. to be accounted for, which must have been spent by the insensible perspiration from the skin and that from the lungs conjointly, on the supposition that the weight of the body remained stationary.

I have already observed that the daily evacuations were not so nearly uniform as was the quantity of food. The urinary secretion was greatest when tea was substituted for milk, and on one day was fifteen ounces above par. On another occasion, finding a greater defalcation than I had before observed, I could discover no cause for it, unless a teaspoonful or two of vinegar taken at dinner could account for it. To be satisfied of this, I took, some days after, an ounce of vinegar in four equal portions during one day; and the effect was a greater diminution of urine on that day, than on any other during the two weeks, the quantity being fifteen ounces below the average, and four ounces less than on the former day when vinegar had been taken. There did not appear to be any increased effect in any other secretion as a compensation for this diminution.

In order to try the effects of different seasons, I resumed these investigations in the month of June the same year, and continued them for one week successively. The results were what might have been anticipated nearly. A less consumption of solids, and a greater consumption of fluids, were observed. The evacuations were somewhat diminished, and the insensible perspiration was increased. The following were the results:—

	Solids consumed in 7 days.	Fluids consumed in 7 days.
	oz.	oz.
	236	391
per day	34	56=90 total,

being four ounces per day less in solids and three ounces in fluids than in the former trial.

The daily averages in the evacuations were, urine 42 ounces—fæces $4\frac{1}{3}$ ounces; leaving a balance of nearly 44 ounces for the daily loss by perspiration, being an excess of about six ounces above that in the former season, or one-sixth more, owing no doubt to the higher temperature of the weather.

Another trial of one week's continuance was made in September the same year. The results were so nearly like those in June as to render an enunciation of them unnecessary. The daily consumption of food was $93\frac{1}{2}$ ounces, and the perspiration one half of that quantity.

I may be allowed perhaps to subjoin one day's experience of the effect that taking a large dose of carbonate of potash (salt of tartar) has upon the secretions. This was suggested by a similar experiment made by Dr. Alexander, and published by him in a small volume of medical essays. His results I do not at present recollect; but my notes at the time imply that I expected the alkali to act as a diuretic. My experiment was made on a fine day at the end of March, after the two weeks' series; the thermometer ranged from 40° to 60°. In the morning I had a basin of tea prepared for breakfast, with the usual quantity of sugar and cream; into this I infused four drachms avoird. (100 grains) of dry carbonate of potash; after it was dissolved, I proceeded to my repast as usual, apprehending the diluted alkali would be so far qualified in its taste by the sugar, as to be rendered tolerably palatable, but in this I was mistaken; the nausea was unbearable; and I was obliged to drink it off as fast as I could, and then eat my toast to an additional cup in the ordinary way. This done, I felt nothing amiss; took a moderate walk and returned. On sitting down I perceived small drops of fluid on the backs of my hands, without any sensation of heat above common. My appetite was rather keener than usual during the day, and I felt uncommon agility in the evening. The secretion by the kidneys was not at all disturbed. But on retiring to bed I burst into a profuse perspiration, which continued through the night, and was felt in degree during the succeeding night. By taking care, the effects went off without any perceptible detriment.

Being satisfied, by the preceding trains of experiments, that no more information was to be expected in this way than was already acquired, I varied the process, with a view to obtain the quantity of perspiration and the circumstances attending it more directly. I procured a weighing beam, by which I could weigh my body, so that the beam would turn with one ounce. Dividing the day into periods of four hours in the forenoon, four or five hours in the afternoon, and nine hours in the night, or from ten o'clock at night to seven in the morning, I endeavored to find the perspiration corresponding to those periods respectively. My method of proceeding was, to weigh myself directly after breakfast, and again before dinner, observing neither to take or part with anything during the interim, besides what was lost by insensible perspiration ; the difference in the weights, in this case, was the loss by perspiration. The same procedure was adopted in the afternoon and in the night.

I continued this train of experiments for three weeks in November, the same year. I then took the aggregate of the morning observations, next that of the afternoon observations, and lastly that of the night observations, and divided each of these three aggregates by the number of hours in the several periods, in order to find the hourly perspiration in each period, apprehending there might be some differences owing to the time of day, or being awake or in sleep. The mean hourly losses, by perspiration, were as under :

Morning, 1.8 oz. avoird. Afternoon, 1.67 do. Night, 1.5 do.

During twelve days of this period, I kept an account of urine, corresponding in time with that of perspiration. The ratio was, urine : perspiration : : 46 : 33, or 7 to 5 nearly ; which is a somewhat greater disproportion than that observed in March—owing, probably, to the temperature of the weather being lower in the latter season.

So far I have given the facts and observations made forty years since. I made no deductions from them at the time ; indeed the knowledge of animal and vegetable chemistry was at that time in its infancy. Since then the progress of this branch of philosophy has been very considerable, and we are now enabled to approximate, in a good degree, to the quantities of the several chemical elements to be found in the great variety of products of the two kingdoms. By combining this knowledge with that obtained from the preceding facts, we may possibly discover or establish some physiological principles important to be understood in the animal economy, more especially in regard to the acquisition and preservation of health.

From the table we have given, it will appear that bread and farinaceous vegetables constitute the greatest part of ordinary food. About the time of the above experiments, I found that five pounds of flour would make seven pounds of bread. Now, from the analyses of flour that are given in our systems of chemistry, I think we cannot estimate the carbone in flour at less than 42 per cent. ; hence we have 30 per cent. of carbone in bread. Twelve ounces of bread (the daily average in the first set of experiments) must then contain 3.6 ounces of carbone. Seven ounces of oat-cake and oat-meal may be estimated, I think, = 1.8 ounces of carbone, or half the quantity that twelve ounces of bread have. Four ounces of pastry can scarcely contain less than one ounce of carbone. Nine ounces of potatoes must contain nearly one ounce of carbone. Four ounces of butcher's meat and two ounces of cheese would have together somewhere about three ounces of carbone, if Gay-Lussac's experiments be nearly correct. Thirty-one ounces of milk, estimating the carbone at three per cent. gives eleven-twelfths of an ounce. Twenty-two ounces of tea and beer would contain only a small fraction of an ounce of carbone, not easily estimated, but of little account by reason of its smallness. From this, it would appear that about eleven and a half ounces of the element carbone is taken into the stomach by one kind of aliment or another in the course of the day in some state of combination.

Chemical analysis has been applied with considerable success to the animal product, urine. According to Berzelius, the urine of healthy persons differs materially according to circumstances. Upon the average it may be reckoned to consist of 93 or 94 per cent. of water, and the rest is a complication of a great many articles. The carbone contained in these ingredients cannot be estimated at more than 1 or 1¼ per cent. from the analysis hitherto made. This will give .5 or .6 of an ounce of carbone upon 48½ of urine per day. Berzelius has not neglected the analysis of the fæces; of 100 parts, three-fourths may be estimated as water, and the rest do not seem to contain more than ten parts of carbone. This would give half an ounce of carbone in five ounces. Hence we may infer that one ounce, a little more or less, of carbone, is carried off from the body daily through these two channels. The remaining 10½ ounces must therefore be spent in the insensible perspiration. The quantity of insensible perspiration from the skin cannot be easily determined by direct experiment. That from the lungs may be approximated from known facts.

I have shown (see Manchester Memoirs, vol. 2d, new series, page 27), that I produce by breathing in the space of 24 hours, 2.8lbs. troy of carbonic acid gas. This is equivalent to .78 parts of a lb. troy of carbone = .642 parts of a lb. avoirdupois = 10¼ ounces, nearly. Now when I estimated the quantities of carbone in the several articles of food, &c. just related, I had no recollection of this quantity of carbone expended in breathing; it may well be supposed, then, that I was highly gratified to find by the calculation that the difference of the two quantities, found by such different modes of investigation, was only one quarter of an ounce.

With respect to the aqueous vapor exhaled from the lungs, I have determined, in the essay quoted above (page 29), that the highest estimate of the quantity I exhale cannot exceed 1.55lbs. troy = 1.275lbs. avoir. = 20½ ounces avoir.; if to this we add 10¼ ounces of carbone, we have 30¾ ounces for the carbone and water expended from the lungs in one day, and this taken from 37½ leaves 6¾ ounces per day for the insensible perspiration from the skin, which, if the above estimate be allowed, must consist of 6½ ounces water, and one quarter of an ounce carbone. According to this, the matter perspired from the lungs is five times as much as that from the whole surface of the body. If, instead of carbone, we trace the element azote into and out of the body, we shall find from our data that from butcher's meat, cheese, and milk, about 1¼ ounce of azote is taken into the stomach daily, and nearly as much passed off by urine and fæces.

Upon the whole we may observe, that of the six pounds of aliment taken in a day, there appears to be nearly one pound of carbone and azote together; the remaining five pounds are chiefly water, which seems necessary as a vehicle to introduce the other two elements into the circulation, and also to supply the lungs and other membranes with moisture. Very nearly the whole quantity of food enters into the circulation; for, the fæces constitute only one-eighteenth part, and of these a part, bile, must have been secreted; one great portion is thrown off by means of the kidneys, namely, about half of the whole weight taken, but probably more or less according to climate and season, &c.—another great portion is thrown off by means of insensible perspiration; this last may be subdivided into two portions, one of which goes off by the skin, amounting to one-sixth part, and the other five-sixths are discharged from the lungs in carbonic acid, and in water or aqueous vapor.

Such are the deductions I have drawn from my early experiments, and from the light which modern chemistry has diffused over the animal and vegetable products. This branch of science belongs more peculiarly to the physician. What the profession may have done in it of late years I am not aware, my studies not having been in that line. But it must be allowed to be a subject worthy the attention of professional characters, and not uninteresting as a branch of general physics.—*Memoirs of the Literary and Philosophical Society of Manchester.*

DELIRIUM TREMENS.

To the Editor of the Boston Medical and Surgical Journal.

Sir,—The judicious remarks of some of your late correspondents upon *Delirium Tremens*, have led me to concentrate my own ideas upon the same subject.

Among the remedies suggested in a late number,* *calomel* is not included; and it is upon this agent that we think in many cases the safety of the patient depends. Our *pathology* of this disease is, that it is a nervous and gastric derangement, produced principally by the cachectic state of the fluids; and that this cachectic state is owing to the introduction of spirituous liquors into the circulating system, by which the whole mass of blood, and consequently the secretions, become deteriorated. The healthy motions of the heart, of the brain, of the nerves, of the stomach, and of the glands, all depend upon the healthy state of the blood; not, however, meaning to assert but that the reverse may happen, and that vitiated fluids may be owing to a derangement of the solids. But in *delirium tremens*, I believe that the blood has alone, or principally, been found in fault, with its secretions. I mean so far as

---

* See a well written paper by Dr. Davis, in No. 17, Vol. VIII.

post-mortem examinations have reached. Hence healthy nutrition is not derived from wholesome food. Nor are healthy motions imparted by the brain to the nerves, nor by the nerves to the muscles ; nor sane sensations imparted by accustomed objects to the mind. Imagination takes the place of sense and reason, and the vitiated eye aids the deception. The gastric juice ceases to dissolve the food and to aid the stomach in the healthy process of digestion, and the appetite is impaired or lost.

The cachectic state of the system is evinced by a full, bloated, purple, red or yellow color of the face and eyes ; and by a mingling of the transparent cornea, by minute, but visible, yellowish, or dirty-red bloodvessels, with the white of the eye. The same state is also shown by eruptions, ulcers, and *gutta rosea.*

When *delirium tremens* invades the intemperate, who have these marks and tokens upon them, the case is more serious, and the prognosis less favorable, than in others who may have been equally intemperate, but who have not these cachectic marks. And it is in this class that small doses of *calomel, combined with opium*, is a remedy very much to be depended upon. It is this bloated, cachectic class, also, which will not bear bleeding, or at least who will not be benefited by it. The calomel should be carried to the extent of producing tenderness and soreness of the gums, without absolute salivation. When a slight ptyalism is effected, the patient may generally be considered safe.

This is the *first* and most numerous class.

There is a *second* class of persons who have *delirium tremens*, however, who have few or none of these cachectic signs ; who are neither bloated, nor red, nor purple, nor very pale. Perhaps the deeds of darkness are most frequent in this class—the mind, rather than the body, displaying the signals which denote the monster Intemperance. The pulse, in this class, is sometimes nearly natural, sometimes rather full and resisting. These patients bear bleeding with benefit.

There is a *third* and last class of persons, who have *delirium tremens*, whose state of system is completely typhoid. The countenance is sallow, pulse feeble, hallucinations melancholy. Sometimes, however, there is no shade of sanity remaining, but a complete oblivion of all the faculties of the mind. When a patient's mental powers are so far obliterated as that he repeats his own name, and screams for himself, or pulls and pinches a particular part of his own body, the case may be set down as a fatal one ; unless, indeed, he live long enough to have a salivation or strangury produced ; and then a cure is not certain. There are two objects answered by a slight salivation ; the one alterative, the other counter-irritant. Both are very important. When, however, counter-irritation is thought indispensably and speedily necessary, and when there is not time for calomel to have its specific effects, *tinct.* of *lytta*, or cantharides in some form, is the dernier resort. The tincture of cantharides answers to two important and urgent indications—*stimulation* and *counter-action.*

But although the three classes of cases to which we have alluded are sometimes definitively marked, still this is not always true. Some cases will occur in which they are combined, and in which the shades, which are sometimes so distinct, run into each other.

The case of J. M. was of this ill-defined class. He was a young man of about 30. Stimulants (with the exception of alcohol), anti-spasmodics, musk, camphor and blistering, did not avail. He was bled with benefit. His hallucinations were hideous. Upon visiting him one morning, he told me that Burke the butcher had been there and sawed off the heads of two of his children. This story he continued to repeat to every one the whole day, and firmly believed it. The sight of his children could not do away this maniacal notion. Blisters and opiates had little beneficial effect. He was, after bleeding, freely and repeatedly purged with calomel and jalap; and then recovered under a tonic course of Peruvian bark and alum. He was never much bloated, nor was his pulse very much depressed, and his case was most nearly related to the second class which we have mentioned. Hepatic congestions give rise to the disease in some; hence the use of calomel, both as a purgative and alterative.

S. B. was also of this class. He had *delirium tremens* in February 1826. His usual quantity of spirituous potation had been a quart a day, by his own account; perhaps more in reality. He would get up and run about the house to catch a goslin which he said he saw, reaching under beds and chairs to catch it, and calling on his son to assist him. In his case there was too much action of the arterial system for either opium or alcohol. Both were withheld. He was twice bled, and took several gentle cathartics. Had a paregoric mixture, combined with a solution of tartarised antimony, and speedily recovered. His age, 63. His health continued so long as he was more abstinent. But in 1831 he had again arrived at his quart a day. He then had every symptom of pulmonary consumption. His purulent expectoration was excessive, his limbs and face were swollen, and the closing scene appeared near. He desisted from strong drinks, and remedies had their effect. After trying the usual routine of remedial agents, I made a thorough trial of *lichen islandicus*. I began without expectation of this or anything else having any curative effect. It however cured him; and it is the only instance that I have known of decided benefit from that once vaunted remedy. I should again resort to it in phthisis, from a similar cause. S. B. afterwards arrived at his quart a day. Since the temperance exertions, his friends reduced the quantity to a pint, as his daily allowance. But with this quantity he was unable to do much labor. The present year it has been further reduced to half a pint. I was lately at his house on account of sickness in the family of his son, and was much pleased to find that he had resumed bodily labor, and that he attended his fine garden, and hives of bees, in good health.

A maiden lady of the first, or bloated class, had *delirium tremens*, with incontinence of urine, in 1830. The latter was the most urgent symptom, and a sore-tribulation, as it was in the winter season, when a wet bed is most uncomfortable. I gave her the *tincture of lytta*, at first in the usual doses, and doubled and trebled the quantity, without any kind of effect. I continued to increase the doses, until they amounted to a tablespoonfull! The specific effects of the remedy now took place; she had strangury, rather severe, but of short continuance, and was cured of the incontinence, and also of *delirium tremens*. She became more temperate, and now enjoys good health. She lost three weeks of her life, as to the recollection of anything that passed. Neither of her maladies have since returned.

In the third, or purely typhoid diathesis of *delirium tremens*, I formerly placed too much dependence upon wine. It will not reach such exhausted and prostrate cases. Alcohol, with sulphate of quinine, bark, and absinthium, the favorite remedy of Dr. Davis, are far more appropriate. But with these, opium, or, which is better, the *sulphate of morphia*, tinct. lytta, capsicum, large doses of musk combined with ammonia, and the oils of peppermint and cinnamon, must be conjoined; whilst the precordia and extremities are excited by blisters, or mustard poultices, or both. Nutritious broths, highly seasoned, and wine toast, by the mouth, or *pro injectio*, are indispensable.

In my earlier practice, I once abandoned a patient of this class, as being in a state entirely hopeless. He recovered, however, under the care of his mother, who kept an apothecary's shop, and who was a great friend to giving opium, elixir paregoric, and liberal diet. She also, I believe, gratified her son in his spirituous potations. I was at that time averse to the latter in such cases, but *experiencia docet*. To his widowed mother, and not to his physician, did this patient owe his life. Blisters, in such cases, are to be applied as *rubefacients*, and not as *vesicatories*, and all evacuations avoided. Yours, very truly,

Lebanon, Ct., June, 1833. JOSEPH COMSTOCK, M.D.

BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, JULY 3, 1833.

MEDICAL EVIDENCE IN THE TRIAL OF THE REVEREND E. K. AVERY FOR THE MURDER OF SARAH M. CORNELL.

WE shall attempt to give to our readers a brief account of the medical evidence adduced in this trial, so far as it involved points of importance to be considered by the practitioner. We do this not simply on account of the interest which is felt in regard to every circumstance in this extraordinary transaction, by every member of the community. There are other reasons. The medico-legal questions which arose in view of the facts adduced, were numerous, and were among the most intricate which the science embraces. They belong to a class of inquiries which, more than any other, are likely to occur on criminal prosecutions, and to be referred to medical men. The decision of these questions, or rather the opinions given in them, are said to have influenced very materially the jury in determining the fate of the prisoner. The examination of Dr. Channing was one of the best sustained expositions of medical opinions to a jury we have ever seen, and those of the other medical witnesses deserve great praise. Yet all were no doubt open to criticism; and by observing where they succeeded and where they failed, all those who are liable to be placed in similar situations will be better prepared to do justice to themselves and to the profession. It was said by an eminent jurist in England, and the observation was quoted recently by one of our correspondents, that medical evidence generally was the worst which was brought up before a jury; the most perplexed, involved and contradictory, and the least useful towards obtaining a clear decision. We shall not at present go into the question how this, if true, is to be accounted for—how far the reproach is shared by the other learned professions, or how far an apology may be found, in the case of the medical profession, in the peculiar difficulty of the questions generally submitted to their consideration. One observation, however, strikes us as important, viz. that if physicians were in the habit of being frequently called on to give opinions on points in medical jurisprudence, these points would be familiar to their minds, and would be stated with more accuracy, clearness, and consistency, than they are at present found to be. In truth, practitioners are seldom called on at all to consider the points peculiarly within the domain of this science, and very rarely to explain them at length, under the embarrassing pomp and circumstance presented by a court of justice. That when brought on the stand, and subjected to the peremptory questions of counsel, even the learned and skilful should commit themselves, is not then very surprising. But since it is on these occasions that the benefit of coolness, self possession, and a familiar acquaintance with the subject, are the most conspicuous; since on the possession and exhibition of these qualities, the life of a fellow creature may at such times mainly depend, it is certainly the bounden duty of every practitioner to acquire such knowledge, and to train himself as far as possible for such examination. It is in this view that we think an analysis of the medical evidence, adduced in the case of Avery, will not be without its interest.

The subject naturally presents itself to us in a threefold division. 1. What facts were sworn to on this trial, in regard to which medico-legal questions could arise. 2. What were these questions. 3. What answers were made to them by the medical witnesses. It will be convenient to consider each distinct fact, with reference to all these points.

I.—Appearances presented by the body. 1. Paleness. 2. The position of the cord on the neck. 3. The depth of the indentation. 4. The marks and discoloration on the abdomen.

II.—Appearances observed on the second examination. 1. Lividity about uterus, removed by washing. 2. Os tincæ open.

III.—Length of the fœtus and its weight.

First, *the paleness of the face* is testified to by several witnesses, particularly Dr. Wilbur, who saw the body before it was removed from the yard. He says, 'the face was not so purple as we usually expect in such cases:' that is, in cases of strangulation. Dr. Hooper says, 'face more pale than I had supposed it would be in a person who was hung.

The main bearing of this testimony is on the point whether death was produced by strangulation, or by some violence previously offered. The question, also, appears to have arisen, whether, if fainting had occurred as a consequence of previous violence, the body being then strangled, would appear more pale than it would do under other circumstances. We shall now see what were the medical opinions given on these points.

Dr. Hooper of Fall River, being asked how he accounted for paleness, said, 'The tightness of the cord might have prevented diffusion of the blood, or the nerves may have been so much compressed as to produce a more sudden death than is usual in strangulation.'

Dr. Parsons of Providence, 'The faces of persons hung are quite as often pale as dark colored.'

Dr. Holmes of Bristol, 'The face of persons hung is usually dark.'

Dr. Dunn of Newburyport, has seen four persons after being hung.

The face is sometimes pale and sometimes suffused. One was as pale as in natural death.

Dr. Channing of Boston was asked, might not the face of a person hung be suffused with blood at first, and the blood disappear in twelve hours. *Ans.* I should think so.—This part of the testimony, then, as may be observed, did not make against the prisoner.

2. The witnesses all state substantially that the cord passed round the neck above the thyroid cartilage. On this point, therefore, it is not necessary to recapitulate the evidence. The question arising on this was, whether such was the usual appearance in cases of suicide by hanging, or whether as likely to occur if death were caused by other means, and the body afterward suspended. On this point Dr. Channing was asked, Where would you expect to find the cord about the neck of a person who had been strangled, and afterward suspended? *Ans.* I should look for it below the thyroid cartilage. *Q.* Would it be difficult in strangling a person, to fasten the cord above that part of the neck. *Ans.* I should think so. On the cross examination—*Q.* Why should you expect to find the cord low down the neck in case of strangulation. *A.* Because it is easier to apply it there, the neck being smaller. *Q.* If the person to be strangled had fainted, or was entirely in the power of a much stronger person, would there be any difficulty in placing the cord round the upper part of the neck, so as to give the appearance of suicide. To this and other questions of similar import, Dr. C.'s answer implied, that under these circumstances he thought strangulation might be produced in this manner.

3. The testimony was nearly uniform as to the horizontnl direction of the cord, and the depth of the indentation. Dr. Hooper says, 'The indentation was three eighths to half an inch in depth. By bringing the head longitudinal, I measured the indentation, and found it an inch and a quarter below the tip of the ear on both sides; was unable to discover any difference in the depth. When the head was erect, the mark was higher behind at the back of the neck than it was in front.' The knot was just below the right ear. There was a slight discrepancy in the testimony as to depth, founded on direct measurement, and that based upon the comparative circumference of the neck in and below the indenture. Dr. Wilbur testified that the circumference of the neck in the indentation was 10½ inches. Just below the circumference was 11½. This difference would obviously give only ⅙ of an inch for the depth of the indentation. The question then was whether these appearance could have been produced by suicide. Dr. Wilbur, in answer to the question, might she with common resolution have drawn a common slip noose, as tight as this was, answers, I think not. The question seemed to be put in this form, because Dr. Wilbur had not, at his examination of the body, noticed the clove hitch. The peculiarity of this knot is, that it does not tighten on being drawn down by a weight, like a common noose. It was therefore a natural inference that the indentation was made previous to the suspension. Dr. Miller, asked, if a rope were so tight as to indent the neck three-eighths of an inch all round, could the person fasten it, so drawn, over a stake, answered no. *Q.* Would the mark round the neck in case of suicide by hanging, be a horizontal line in the ordinary course of things? *A.* It would not. I should think it rather natural than otherwise, that the mark should be horizontal in throwing the head back in cases of suicide. We can hardly tell. These things change. My answer is, I don't know. *Q.* Would not the indentation opposite, be deeper than on the side where the rope passes up. *A.* In case of an ordinary knot it would.

Dr. Parsons being asked the same questions, answered, 'The stricture making an indentation three-eighths of an inch, would nearly stop respiration. If equally deep all round, it would stop sensation in less than a minute. Respiration might continue after sensation was gone. In ordinary cases of hanging, the mark round the neck is not horizontal.'

Dr. Dunn of Newport, in reply to the question, how long consciousness would continue after respiration was interrupted, remarks, 'A cord making an indentation of one-eighth of an inch, would probably produce immediate stoppage of the circulation in the principal arteries. The head is supplied by two arteries, which are so defended by bones that they could scarcely be strictured. It is difficult to solve this question (as to sensation) in a satisfactory manner.'

Dr. Channing, asked, Could a person, after drawing a cord about the neck so as to make an indentation one-eighth of an inch, fasten the other end to a stake. *A.* I should think not. *Q.* Would it stop the circulation? *A.* I should think not. *Q.* How then would death be produced in hanging? *A.* By pressure on the windpipe, stopping respiration. Respiration is the function that must be suspended to produce death, and it is that on which sensation depends.

We have forborne to make any remark on this part of the testimony till the whole was presented in one view. The witnesses differ somewhat, as will be seen in the order they assign to the symptoms which precede death. Dr. Parsons, we observe, thinks that respiration might continue after sensation was gone. How this could happen in hanging, is not clear. As respects the bearing of the facts on the question of suicide, we see no very material diversity of opinion. All seem to think the facts inconsistent with the idea of suicide by suspension.

4. Dr. Hooper states that on Monday, three days after death, he examined the body, which had then been buried and again removed. Abdomen on right side was discolored and livid. On left side, above hip, mark of contusion. The women who laid out the body testify to prints of fingers on both sides of abdomen, low down, one side marks broad, the other as if fingers were pressed. The *parts* much bruised and some blood. These prints pointed upward. Some appearance on the linen beneath, of vaginal discharge. One witness also testified to two marks on back below shoulder blade.

The questions then arose how far these marks were proofs of violence, or whether part might have been produced by putrefaction, and part by the action of removing the body.

Dr. Miller.—'It is common for persons after death to exhibit dark appearances about the privates. The lower regions of the abdomen, *it is supposed*, after death, are discolored sooner than others. The simple dark appearance on the skin, which follows sooner or later after death, is not conclusive evidence of bruises. A person suspended for some time by the neck would exhibit darker appearances at the lower part of the abdomen, than if laid on a table.'

Dr. Turner—'Livid appearances about the back and genital organs are very common, and may be mistaken by ignorant persons for bruises. The effect of pressure on a body, after death, would be to render the parts pressed less livid than the surrounding parts.' Dr. Dunn to the same effect.

Dr. Channing, asked, 'Is it common for persons of both sexes to exhibit dark appearances about the lower regions of the abdomen after death?' *A.* Very. *Q.* To what extent. *A.* To an extent that would strike one not accustomed to it as remarkable—an obvious discoloration. In dissections I have made, the uterus and vagina were generally found discolored, the lower part especially, the body and fundus not so much. The orifice and neck of the uterus have been more or less discolored. Such has been the case in most of the dissections I have met with. The exceptions, if any, I have not borne in mind. The pendant and interior parts have uniformly been discolored, after natural death. The natural red color of these parts, which is brighter in the young than in the old, is changed after death, by the color of the blood becoming deeper—bluish and black. It is a change of the natural red color to the dark color it falls into after death. The blood undergoes this change throughout the system, but it is more obvious in the parts of the body referred to, in consequence of the natural color being deeper there, and more marked, before death. Those who die a natural death, very soon after exhibit livid spots in various parts of the skin, and especially the parts that are lowest, whether in a horizontal or perpendicular position; such as the back, side, hips, &c. These marks are dark, blue, and livid; and contrasted with the paleness of the skin in surrounding parts, have an appearance of great discoloration. This occurrence is familiar to those who attend upon the sick, just before death. It begins in the fingers, &c. It is more remarkable after death. It is a consequence of the subsidence of the blood; its falling into those parts which are the lowest, by its own weight. *Q.* What are the appearances you would expect to find in those parts, after a person had hung suspended fourteen hours after death? *A.* I should expect to find them discolored, dark and livid, exhibiting the appearance that the back does when the body has laid horizontally after natural death. I should look for a stronger and more decided discoloration there, than I ordinarily should on the back in natural death. I think they may be mistaken for bruises by people ignorant of such appearances. *Q.* If there were anything like contusion before death, would the discoloration be comparatively darker? *A.* In the centre it would be darker, and somewhat in its neighborhood. *Q.* If there were indentations on the abdomen, could they have been made before death? *A.* I should think not.

In the cross examination, Dr. C. underwent a series of interrogatories with the view of determining how far women accustomed to seeing bodies, would be capable of distinguishing the marks of bruises from those produced by other causes. For these we refer to the trial. The following queries are more important.

Q. Would a person taking down a body be likely to make visible marks on the flesh? *A.* I should think they might. *Q.* If, before death, pressure were made by the hands on a body, would the settling of the blood be more distinctly marked there than elsewhere. *A.* I should think not. *Q.* Why. *A.* The pressure being made on the yielding walls of the abdomen, I should think no extravasation of blood would follow.

It is not easy to determine the inference to be obtained from this testimony. The prints on the abdomen referred to as indentations by the prisoner's counsel, were described by the witness as marks. Whether so or not they are equally unaccounted for. In either case Dr. Channing thinks they could not have been made before death, and in either case we do not perceive how they could have been made after death. The supposition of their being produced by the taking down of the body, which at the time was covered with its usual clothing, is attended with some difficulty. Moreover we are unable to perceive how the hands could have been applied in a manner to produce a print in this direction. Possibly the evidence itself of the fact, might have been regarded as not sufficiently consistent to deserve entire confidence.

II. Secondly, we come to the facts which appear in evidence in regard to the second examination, made, as it appears, 36 days after death. The object of the exhumation was, expressly, to discover the truth of a rumor which prevailed, whether the arm was broken, and the other appearances were noticed incidentally as they presented themselves.

Touching these appearances, then, Dr. Hooper testifies, that 'the face was covered with a white mould, where it had been touched by the frost. The blood flowed from the veins, an dthere was no smell. On the muscles of the abdomen, lower down, we found bruises; extravasated blood. Likewise found on the back above pelvis, on the superficial muscles, bruises, perhaps two inches over on each side. The extent of the bruises in front, above the bones of the pelvis, appeared six inches long, almost from one groin to the other. Mouth of uterus nearly black; os tincæ open. Vagina of a dark livid. It presented a different color from the intestines. We tried the test of washing. The mouth of the uterus washed white. Other parts of the intestines, when washed, could not be made white. Also the bruised muscles were washed with the same result [of becoming white?]. Again, in dissecting the muscles we found them perfectly fair, except in this place, where the clots required to be removed by forceps.'

The question arising in view of this evidence, is, how far lividity of the muscles and mouth of the uterus, not washing out, and the os tincæ being open at the end of thirty-six days, were proofs of violence committed before death, in order to produce abortion, and how far these appearances were attributable to other causes. We have preferred to connect the two classes of facts in this inquiry, as several of the considerations affecting their importance apply to both.

Dr. Hooper says, 'I should draw no strong inference from the uterus being open at the end of thirty-six days, and discolored—not from that fact alone.' Again, 'At the second examination I was satisfied that there was one bruise at least on the abdomen, but am still of opinion that some of the discoloration was owing to the change that had taken place.'

Dr. Wilbur being asked, Could you swear to your belief that any hard substance had been introduced into the uterus to procure abortion, from anything you saw on the examination of the 26th? *A.* I could not [otherwise] account for the discoloration. *Q.* Can you account for the opening of the uterus aside from the discoloration, without the introduction of a hard substance? *A.* I do not know.

Dr. Miller.—'The os tincæ being open, and the surrounding parts discolored, thirty-six days after death, I should consider as affording no certain evidence of violence. The os tincæ is hard, and I should think it would be the last organ to go into decay. If abortion were attempted by one not skilful, it would be difficult to reach the os tincæ. [Witness then described the locality and the reasons for this opinion, which are familiar to medical men.] In this case, as I understand, the fœtus was not disturbed, which shows the attempt was not successful, if made. It is dangerous to rely on any testimony, at that length of time, respecting those organs. Upon all the appearances described, I could not come to a satisfactory result by examination 36 days after death. I am but little acquainted with the test of washing the muscles. Perhaps my opinion would not differ [from Dr. Hooper], what little I know about that test. Gangrene will not wash: but if there was any bruise before death, it would wash out. *Q.* What inference would you draw from finding coagulated blood, under such an appearance, 36 days after death, which required to be removed by the forceps? *A.* I should consider it an evidence of injury received before death. Coagulated blood never appears in sugillation. It may be present in ecchymosis. There is no way to discover an ecchymosis satisfactorily, but by opening at the time.'

On cross examination, Dr. M. says, 'The reliance to be placed on dissection 36 days after death would, to be sure, depend on the degree of preservation of the body. Nothing but decay would change the result of an examination between one day and 36 days.'

Dr. Parsons.—'I should place no dependence on the appearances in the private organs or in the back 36 days after death. I should expect these appearances, whether there were bruises or not.......The uterus being open at that period, would not be a circumstance I should depend on to prove that a hard instrument had been introduced ; nor should I upon the livid appearance described in that organ.'

Dr. Turner.—If there were mere discoloration of the parts, I should not infer violence. If there were ecchymosis to any extent, I should infer violence. The washing of the blood from the muscles after such a lapse of time would not enable me to form any satisfactory conclusion as to the probability of violence done to the parts. The appearance of the uterus, as described, would furnish no satisfactory proof of violence. If there were an effusion of coagulated blood under the skin, I should infer violence. If the ecchymosis were to a very great degree, so as to make a considerable body of coagulated blood, I should infer violence 36 days after death.'

Dr. Dunn.—'I might form an opinion from the appearance of bruises, 36 days after death, but should not be willing to swear positively unless there were marks of laceration or fracture......It is familiar, in dissecting rooms, that a body procured in the winter will be preserved much longer in the same temperature than one procured in the summer.'

Dr. Channing.—Asked, Can any satisfactory results be arrived at 36 days after death, to determine bruises ? *A.* I should think not, unless there were lacerations or dislocation. *Q.* After 36 days interment, if you were to find dark spots on each side of the back bone, above the pelvis, together with coagulated blood, the body having previously been opened, could you draw any satisfactory inference as to a bruise before death ? *A.* I could not. *Q.* Suppose you found coagulated blood which required to be removed by the forceps, should you consider it a satisfactory assurance of a bruise before death ? *A.* I should not. Again being asked, Would not your opinion as to the reliance to be placed on an examination depend upon the state of preservation the body was in, and not upon the lapse of time ? *A.* I should say no ; but with this qualification —if the brain and hard parts of the body were to be examined, I should expect to find them the same ; if the soft and exposed parts, which are so liable to undergo changes, I should look for a change. In either case, I should not place much reliance on an examination 36 days after death.

It seems, then, to have been admitted by all the witnesses, that the state of the os uteri might be explained as an effect of the relaxation and softening of the parts which would occur after death. As respects the other appearances, there seems not to have been the same coincidence of opinion. These appearances consisted in a lividity, approaching to blackness, of the mouth of the uterus, which by washing became white ; of a bruised appearance of the muscles of the abdomen and back, which, if we understand the testimony, likewise lost their color by washing ; of a discoloration less marked of the intestines, which washing would not remove, and of coagula lying on the muscles above the pelvis, which required to be removed by the forceps. In what manner these circumstances were explained by the physicians who examined the body, has already been observed, and with them Dr. Miller seems to have agreed in regard to the essential points. Dr. Channing, on the contrary, is of opinion that the aspect of the parts, when washed, at the period assigned, affords no satisfactory evidence as to the cause of the discoloration; and thinks that the coagula in question might be produced by the blood extravasated at the first examination, which by its specific gravity had found the lower part of the abdomen. It must be confessed to be a remarkable circumstance, that if these marks of violence existed, they should have failed to attract notice at the first examination; and this defect in the evidence against the prisoner did not fail to be availed of to its full extent by his able counsel.

III. We come, thirdly, to a consideration which will not long detain us—the size and weight of the fœtus, as compared with the time from which the deceased had dated her conception, and with the testimony of her sister as to the cessation of the menses. The alleged date of the conception was the 30th of August. The last appearance of the menstrual flux was August 21. Death took place the 20th of December. The fœtus weighed 5 oz. and measured 8 inches in length. The question arising upon these facts was the following: whether it was more probable that a fœtus of three months and twenty days should have attained the size and weight already mentioned, or that menstruation should continue after conception had taken place. If the latter should be shown to be by far the most probable, it would go to prove that the conception did not take place at the time alleged, and to remove the principal argument in favor of the paternity of the prisoner.

As respects the relation of the size of the infant to its age, very little was stated by the medical witnesses on their own authority: we shall therefore omit any formal repetition of this part of the evidence. It appears generally that the measurements assigned by different authors for the same age vary considerably. The largest estimate of length at four months is that given by Beclard, and is 8 inches. That of Gardieu, who gives the smallest, is 4 inches. Orfila assigns 6. Dewees and Burns make it 5 inches. The measurement of Beclard, therefore, is the only one which corresponds to that supposition which would criminate the prisoner, and this differs so considerably from the average of opinions on this point, as justly to have been regarded with some suspicion.

Touching the point of menstruation after conception, Dr. Miller says, 'It is not very uncommon for females to have some show of periodical occurrences after gestation commences.

Dr. Turner.—'The signs of pregnancy are very equivocal. A female who knew she had cause for it, would suspect herself on the non-recurrence of the periodical discharge.'—Nothing peculiar was stated on this subject by the other witnesses, and nothing definite suggested or quoted as to the proportion of exceptions to the general rule.

We have aimed at nothing more, in giving this sketch, than to present, in the words of the witnesses, the leading traits of the evidence on points essentially within the province of medical jurisprudence, together with such facts as were necessary to render this evidence intelligible. For many details, therefore, calculated to be highly interesting to the profession, we must refer our readers to the whole report.

NOTICE.—Our next will be a double number, and published a fortnight from to-day. We regret the delay in offering communications intended for the present number. Its limits have also excluded some remarks on the evidence, which were intended for the sequel of the above sketch.

Whole number of deaths in Boston for the week ending June 29, 18. Males, 10—Females, 8.